Ice formation in a large body of water.—In view of the possibility that the ice formed at the bottom of a river (the anchor ice proper, as mentioned at the beginning of this paper) may be owing in part to some other cause in addition to the high emissivity of the river bottom, it is desirable to call attention to the following peculiar formation of ice as the result of cooling and refrigeration in a body of still water.

In test D, already described, the receptacle containing the water was a wide-mouth bottle, 6 cm. in diameter at the top, and about 14 cm. in height. The bottle was wrapped in hair felt and tightly packed in a box. Cooling was therefore mainly by conduction and radiation from the top. On one occasion it was observed that instead of the ice freezing solid across the top, there were several extremely thin "curtains" of ice suspended in the water as shown in figure 7. They were visible only in intense sunlight. Several thin, narrow, saw-toothed blades extended far down inside the bottle. They seemed to take the line along which refrigeration occurred as a result of cooling, principally by conduction and radiation at the top of the vessel. It would be desirable to determine whether the density of these fine ice spicules is the same as that of a large mass of ice.

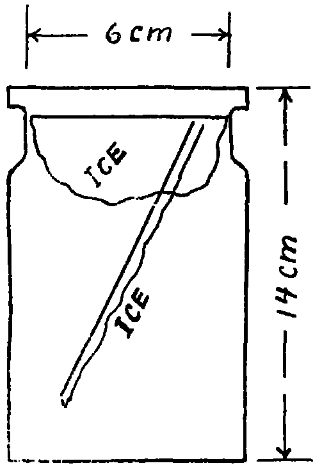

FIG. 7.—Test D with wide-mouthed bottle, showing "curtains" of ice suspended in the water, and saw-toothed blades extending far down into the water.

SUMMARY.

This paper deals with the formation of ice fringes upon the Dittany, *Cunila mariana*. The data here presented are based upon experiments and observations in the field and in the laboratory.

It was observed that the ice fringes are formed when the temperature falls to freezing; but they are not a function of the hoarfrost which may be present upon the ground.

The ice fringes do not form upon the side of a splinter which contains the pith or upon the line of fracture, but upon the outer woody surface. The formation of the ice fringe, however, is not a function of the surface condition of the stem. The stem is frequently found to be cracked, but usually no ice protrudes from the rifts.

The growth of the ice fringe ceases when the ground is frozen to a depth of 2 to 3 cm., and when the moisture in the stem is frozen.

The dimensions of the ice fringes and the height to which they extend above the ground depend upon the rate of evaporation from the stem, and upon the amount of moisture in the ground. Over 5 grams of ice may be formed upon a single plant during a single night.

Photographs are given of ice fringes formed upon stems which had been kept in the laboratory several months. They show that the ice may be formed upon stems without the roots. Hence the ice is not formed as a result of hydrostatic pressure exerted by the roots, which are perennial.

All the observations agree in showing that the moisture rises in the stem as the result of capillary attraction. The height (1 to 5 cm.) to which the moisture can rise within the stem is governed partly by the rate of evaporation from the surface.

Photomicrographs of thin sections of plants are given, which show the structure of stems of plants which do not form ice fringes; also photomicrographs of sections of stems of plants which form ice fringes. It is shown that those plants which form ice fringes the most readily and in the greatest abundance have the most sap tubes.

The ice fringe is a composite of a number of very thin ribbons. In the laboratory the formation of the ice fringe was observed from its very beginning. The first stage in the production of the ice fringe consists of a single row of fine hairlike filaments of ice. This row of ice filaments lengthens up and down the stem. The filaments increase in number, thus forming a solid wedge-shaped "tooth" of ice, which constitutes the second stage of development. In the third stage of development the wedge-shaped tooth of ice widens and increases in length, as the result of freezing of the water which continues to soak out of the stem.

There appears to be no difference between the formation of these ice fringes and the "ground ice" formed on wet soils other than that in the latter a particle of gravel usually forms the nucleus to start the congelation. In both cases the moisture is brought to the surface by capillary action. When the rate of supply to the surface is more rapid than the loss by evaporation, and the air is at a sufficiently low temperature, ice is formed.

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ARE LIGHTNING FLASHES UNIDIRECTIONAL OR OSCILLATING ELECTRIC DISCHARGES?[1]

By CHARLES F. MARVIN, Chief.

[Dated: Weather Bureau, Washington, D. C., June 13, 1914.]

L. A. De Blois, in making experiments upon electrical discharges in a wireless aerial (natural period 400,000) caused by neighboring lightning flashes, using for this purpose both a static voltmeter and an oscillograph whose behavior was photographed or examined by means of a revolving mirror, found only nearly instantaneous lateral flicks of the oscillograph needle. In some cases the needle showed a number of minor oscillations executed during the return of the needle to its zero position. The duration of a discharge of this kind was at least 0.0034 second. The periodic time of the oscillograph was 5,000 to 6,000 per second.

De Blois states that—

In no records was there any indication of regular periodic high-frequency oscillations in the induced current. Had they existed, the

[1] This paper and the following comment refers to the preliminary edition of the paper by L. A. De Blois, "*Some investigations on lightning protection for buildings,*" presented at 294th meeting Am. Instit. Elect. Eng., Washington, Apr. 24–25, 1914, and appearing as Trans. Am. Instit. Elect. Eng., 1914, **33**:563–579, and pl. 25. The illustrations of that paper are not reproduced here.—EDITOR.

oscillograph, of which the periodic time was 5,000 to 6,000, would have failed to respond. This was never the case with heavy discharges to earth near by, though it did happen,[2] presumably for other reasons, with many purely intercloud discharges. Moreover, the static voltmeter could hardly have been expected to respond to high-frequency oscillations as it did in the majority of cases of lightning discharges to earth. On the other hand, it is probable that the currents induced in the collector aerial followed in a general way at least the variations in the traveling waves of the lightning discharges, since such variations, judging by the oscillograms, involved high values of *di/dt* only in the case of steep-front waves, and also for the reason that the damping effect of the aerial, whose natural period of vibration was about 400,000, must have been very small. If such was the case, do not the records obtained point to the fact that lightning discharges are essentially unidirectional, nonoscillatory, aperiodic phenomena, especially since

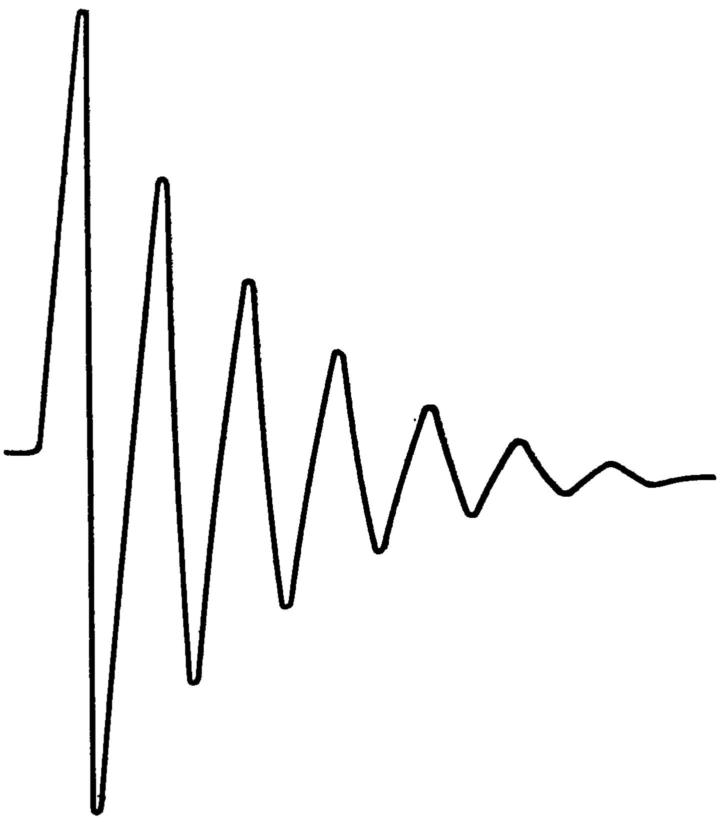

FIG. 1.—Illustrating damped oscillations.

the steep wave-front occurring in the majority of such discharges would account for all the usual effects attributed to high-frequency oscillations?

While in the passage quoted the author asks the question if the observations cited do not indicate that lightning discharges are unidirectional, nevertheless elsewhere in his paper it is plain he concludes that the results seemingly "indicate that such currents *are* nonoscillatory and aperiodic."

The present writer is of the opinion that the behavior of the static voltmeter and also the oscillograph have been misunderstood and that the records and actions described by De Blois are not at all inconsistent with the assumption that lightning discharges may be the high-frequency periodic currents they have been commonly supposed to be. The writer does not mean to affirm that lightning discharges are necessarily oscillatory, but that De Blois's experiments, fairly interpreted, indicate that such discharges may be oscillatory, rather than aperiodic, as he concludes.

It is well known that the duration of a lightning flash is but a small fraction of a second. This is conclusively shown by De Blois's results. For the purposes of demonstration we may assume that a lightning discharge *is* an oscillatory current of high frequency, as has been claimed or admitted by a number of eminent authorities.

We must regard this periodic flow as very strongly damped, and if we had a proper oscillograph to depict such a current it should give us a familiar record of damped oscillations, as shown in figure 1.

During the whole interval of time represented by the first half-wave of the discharge, the oscillograph will be influenced by a powerful electrical surge that will be adequate to cause just such a sudden "flick" of the needle as described and recorded by De Blois. If the period of the damped train of waves was very much greater than the comparatively slow period of the oscillograph, only 5,000 to 6,000, we believe the oscillograph could show only a relatively feeble response to the first great surge of the discharge and would be unaffected by the later relatively feeble waves of the train. If, however, the frequency of the lightning discharge was relatively moderate, but still much greater than that of De Blois's oscillograph, may we not reasonably say that the first surge of the damped discharge would, as before, "flick" the needle to one side, while the subsequent train of waves would cause the minor oscillations observed to occur in some cases? It is quite conceivable that the minor oscillations seen by the aid of the revolving mirror may represent something in the nature of "beats" between the oscillating discharge and the vibrating system of the oscillograph.

The reasoning followed in the foregoing is applicable to the interpretations of the indications of the static voltmeter, and we believe the conclusion is justified that the results obtained by Mr. De Blois from the oscillograph and static voltmeter, in the study of lightning discharges, is not inconsistent with the supposition that such discharges are of a high-frequency damped periodic character.

UNITED STATES WEATHER BUREAU,
Washington, D. C., June 9, 1914.

CHIEF OF BUREAU:

SIR: My delay in replying to your request for a discussion of your note on the lightning flash was caused by a long wait for some promised data from the Bureau of Standards that bears on the subject.

The oscillograms obtained by Mr. De Blois indicate, it seems to me, that the electric discharges that caused them were either wholly unidirectional or else so strongly damped as to be prevailingly in one direction. The relatively long duration, 0.0015 second, roughly, of the individual discharges seems to preclude the idea of strong damping and, therefore, the assumption of oscillation of any kind.

There are several other arguments against the assumption that lightning discharges are alternating, among them the fact that oscillations are impossible if the resistance of the path is greater than about 200 ohms per kilometer. Such scanty information as I have been able to find on the resistance of flames and electric arcs indicates that the resistance of the lightning path probably is much greater than this limiting value.

Respectfully,

(Signed) W. J. HUMPHREYS,
Professor of Meteorological Physics.

The above remarks were also submitted, with a copy of Mr. L. A. De Blois's paper, to the Superintendent of the United States Naval Radio Service, and through him to the United States Bureau of Steam Engineering. The comments of that bureau are as follows:

[Second indorsement.]

UNITED STATES NAVAL BUREAU OF STEAM ENGINEERING,
August 8, 1914.

Subject: Lightning flashes, whether unidirectional or oscillatory.

1. The bureau has carefully considered the question submitted, but a press of urgent matters has delayed reply, and the same difficulty now

[2] We infer the author's expression "it did happen," is intended to mean the oscillograph failed to respond.—C. F. M.

stands in the way of a discussion at length. The bureau's general conclusions in their most conservative aspect can not be better expressed than in the words of Mr. Marvin in the paragraph beginning on page 500 "The present writer is of the opinion," and ending * * * "rather than aperiodic, as he concludes," of his paper attached, wherein he concludes, practically, that De Blois' proposition is not proved. With the whole of Mr. Marvin's paper the bureau concurs, and especially with the ingenious suggestion as to the possibility of something in the nature of "beats" between the oscillating discharge and the vibrating system of the oscillograph.

2. The description of De Blois's experiments leaves the bureau uncertain as to the exact arrangement of apparatus with which the oscillogram in figure 3 [not reproduced from De Blois] was obtained. If this record was obtained from a spark discharge *from the antenna* described on page 564 when excited by the oscillation transformer described on page 570, the bureau would be inclined to believe that his experiments indicate positively the probability of an oscillatory rather than a unidirectional character of lightning discharges.

3. The antenna described is a strong oscillator. De Blois himself speaks of it as having low damping. Its tendency, therefore, upon discharge is to effect this discharge through a long series of oscillations in its own natural period, irrespective of the method of excitation. If excited by impact it follows this tendency without hindrance. A unidirectional lightning flash would appear to afford ideal shock excitation, and since we know that the observed spark from the antenna is oscillatory in such a case the question arises "What is the behavior of the oscillograph under this condition?" This point appears to have been neglected in the experiments, though seemingly it might easily have been investigated by means of ordinary quenched gap excitation. Suppose we have investigated it and have found the oscillograph to give *only sudden definite flicks* like those in figure 3 [not reproduced from De Blois]. How then explain the succession of peaks such as those actually observed, as shown in figure 1 [not reproduced from De Blois]? Obviously either—

(1) As the result of "progressive breakdown" phenomena, described by Steinmetz, in which case the demonstration is still conclusive, or

(2) As the result of forced oscillations of long period or of mixed period, which is the same as saying that the lightning flash is oscillatory.

4. It appears to the bureau that nothing is to be deduced with certainty from this data until the behavior of the oscillograph is known when subjected to oscillatory discharges of different frequency and damping. With an instrument of such low periodicity as 5,000—6,000 too many unknown factors enter to allow a reasonable attempt to interpret the results except in the light of such experimentation.

Bureau of Steam Engineering.

THE ATMOSPHERE OF THE PLANET MARS.

The subject of the so-called canals on the planet of Mars has been carefully treated in a series of articles by Prof. William H. Pickering, published in Popular Astronomy in 1914. He finds that one may perceive similar canals on the moon if only its surface is examined under the same optical conditions used in the study of Mars. It may be worth while for the meteorologist to study the surface of the earth under similar conditions. He has only to represent on an artificial globe the portions that are water, snow, or ice; mountains, plateaus, or lowland; forests, prairies, or deserts, by a proper system of coloring; then place the globe at the proper distance and view it with the proper magnifying power through an atmosphere that is more or less hazy or undulating. The following extracts from Prof. Pickering's paper will show what could be expected under these conditions:

LUNAR CANALS.

The present report will be devoted chiefly to a study of the canals. By a canal in the astronomical sense of the word is meant any long, narrow dark marking that is straight, or of large radius of curvature and of fairly uniform breadth and density. The existence of the canals on Mars as objective realities must appear obvious to any one who has seen the planet under sufficiently favorable circumstances. They have been seen at this station [Mandeville, Jamaica, W. I.] with the 11-inch refractor by using a magnification of 660 when the diameter of the planet was but 5.6 inches. When well seen they can be held by the eye like any other real marking for indefinite periods. The main cause of the controversy regarding them is that in northern latitudes, where most of the large telescopes are located, the seeing is not sufficiently good to show them well, and their existence therefore continues to be doubted in some quarters.

Another cause of doubt depends on the mistaken idea still held by the public, and also by many astronomers, that the larger the telescope the more you can see with it. If the seeing were good enough, or if the objects were very faint, this would obviously be true. But even with double stars there is a limiting size of aperture giving the best results, depending on the quality of the seeing, and with bright planetary detail this limit is very marked indeed. The statement once made in joke that the 40-inch Yerkes lens is too powerful to show the canals of Mars is literally true. There are too many air waves constantly passing in front of its great surface to permit of the necessary planetary definition.

For our northeastern States the best results on a good night can be obtained with an 8-inch aperture. In the Southwest 16 inches is perhaps the limit. The proper size for the Tropics has not yet been reached by any instrument located there, and is still unknown. Any telescope intended for regular use on planetary detail should be provided with a cat's-eye diaphragm placed over the objective, which can be adjusted instantly from the eye end of the telescope, a device first used I believe at the Lowell Observatory.

As a means of studying the canals of Mars it occurred to the writer that since the moon is a body closely resembling it, and of the same order of size, if we were to apply to it the same magnification in proportion to its distance, that we might get a similar effect. The average distance of Mars at opposition is 50,000,000 miles, the average distance of the moon, one-quarter of a million. The moon being at one two-hundredths the distance of Mars, we should use one two-hundredths the magnification. The Martian canals are well seen with a power of 500. An ordinary opera glass giving a power of 2.5 would therefore be a proper instrument with which to view the moon. If we wish to have the moon appear of the same size as Mars, a field glass magnifying five times should be employed.

A preliminary sketch made with an opera glass on April 28, 1912, showed a number of long, narrow canals crossing the face of the moon. Of these the most conspicuous was the broad, double canal shown in figure 1 [not here reproduced] extending northwesterly from Tycho. Next came the three canals to the west of it and a few of those shown in Imbrium. All were narrow and quite uniform in appearance. The present sketch was made with a field glass magnifying four times, on October 14, 1913, colongitude 90°. The double canal extending from Tycho was now so broad that it had almost lost its canal-like character, but numerous fine canals appeared, among them several exceeding 400 miles in length. Only the more conspicuous ones have been drawn, in order to avoid confusing the sketch. When near the terminator the canals are faint or invisible. They seem to be most conspicuous when the moon is full, and individually to vary more or less with the colongitude. It would seem as if a detailed study of them might repay the careful observer. Perhaps the best results are obtained on a slightly hazy evening or with the moon not very far above the horizon, so it shall not be too dazzling. Even with an opera glass better results are obtained if it is steadied by holding it against a post. It is perhaps easier to see them in the first place with a field glass, but once seen, an opera glass gives them a more canal-like appearance. With the latter they are narrow, straight, and gray or black; with the former they show a slightly irregular structure, and at times are distinctly brownish. There are few canals on Mars more distinct than those between Copernicus and Aristarchus when seen under favorable conditions with an opera glass.

But it is not necessary to wait for a moonlight evening in order to see the lunar canal. The same result can be obtained, though in an inferior way, from any good photograph of the full moon. This should be placed against the wall in a strong light and viewed with the naked eye from a distance of between 20 and 40 times its diameter. If we get nearer than this we see that the lakes, with the exception of Plato, are not sharply defined regions, but simply small, dark areas of irregular shape and density, which are in reality much larger than they appear in the drawing. Similarly the canals, which are drawn as heavy, fine lines, are in reality broader and less intense areas of the same length. They vary in general from 20 to 60 miles in breadth. The effect is clearly not due to areas of irregularly distributed and imperfectly seen fine detail. The surface is not necessarily irregular in density nor spotted nor filled with any detail at all. All that is required to produce a canal is a comparatively slight difference in density, a reasonable breadth, and a sufficient intrinsic brilliancy to render it visible. * * *

A study of the lunar canals calls to our attention an unlooked-for characteristic. We should naturally expect that as we approached nearer and nearer to the photograph, or used higher and higher powers upon the moon, that while the canals first seen would be resolved and exhibited in their true aspect, that other finer canals would appear which a closer approach would in their turn resolve. On the moon, excepting with very low powers, this does not seem to be the case. It is true that with a power of several hundred diameters, short, uniform canals make their appearances, resembling in all respects the short,